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THE JOURNAL
OF
THE ROYAL ASTRONOMICAL SOCIETY OF CANADA

DEVOTED TO THE ADVANCEMENT OF ASTRONOMY AND ALLIED SCIENCES

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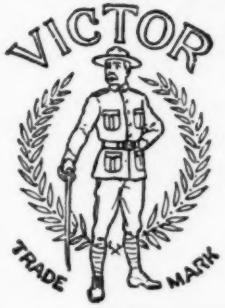

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CONTENTS OF VOLUME I.

I. JANUARY–FEBRUARY.

Introduction - - - - - - - 1

Progress in Astronomy and Astrophysics in 1906 - - *C. A. Chant* 2

List of Papers and Lectures, 1906 - - - 20

Treasurer's Report - - - - - *George Ridout* 21

Astronomy as a Science - - - - - *W. F. King* 22

Magnetic Disturbance and the Aurora - - - *R. F. Stupart* 38

A remarkable Aurora on August 7, 1906 - - - *J. McEachren* 42

A Magnetic Disturbance Synchronous with the Aurora - *R. F. Stupart* 43

A Case of Globular Lightning - - - - 44

The Spectrum of *Mira Ceti* - - - - *J. S. Plaskett* 45

The Ottawa Section of the R.A.S.C., 60; The Society's meetings at Toronto, 64; The Astronomical and Astrophysical Society of America, 67; Notes from the Dominion Observatory, 69; Brief Astronomical Reviews, 72; Astronomical News, 74; Summary of Weather, December and January, 77.

II. MARCH–APRIL.

In Memoriam - - - - - - - *T. E. Heath* 81

Agnes Mary Clerke - - - - - *Elsie A. Dent* 81

Time Service at the Dominion Observatory - - *R. M. Stewart* 85

Adapting a Universal Spectroscope for Radial Velocity Determinations - - - - - *J. S. Plaskett* 104

Magnetic Disturbances Recorded at Agincourt Observatory *R. F. Stupart* 122

Thoughts on Meteorology and Terrestrial Magnetism - *Andrew Elvins* 125

Meeting of the Society at Toronto *(E. A. D.)*, 127; at Ottawa *(J. S. P.)*, 129; Notes from the Dominion Observatory, 133; Brief Astronomical Reviews, 138; Errata in *Handbook for 1907*, 139; Astronomical News, 140; Summary of Weather of Canada for February and March, 141.

III. MAY–JUNE.

Some Cosmical Aspects of Radioactivity - - *E. Rutherford* 145

The Astronomy of the Bible - - - - *J. E. Maybee* 166

Tides of Lake Huron - - - - - *W. J. Loudon* 181

Notes on Double Stars - - - - - *T. E. Espin* 186

The Telescope - - - - - - *D. B. Marsh* 188

Audibilily of the Aurora - - - - - *W. F. King* 193

Meetings of the Society at Toronto *(E. A. D.)*, 195; at Ottawa *(J. S. P.)*, 199; at Peterborough, 203; Notes from the Dominion Observatory, 205; Astronomical Notes, 209; Summary of Weather of Canada for April and May, 210.

IV. JULY-AUGUST.

Variation in the Leading Features of the Tide in Different Regions - - - - - - - *W. Bell Dawson* 213

Errors of Transit Observations - - - - - *R. M. Stewart* 228

The Spectroscopic Binary α Draconis - - - *W. E. Harper* 237

Work at the Lick Observatory and Improvements in its Equipment - - - - - - - *C. A. Chant* 246

Notes from the Dominion Observatory, 264; An Appeal from the Astronomical Society of the Pacific, 268; Astronomical Notes, 269; Summary of Weather of Canada for July and August, 270.

V. SEPTEMBER-OCTOBER.

The Astronomy of Shakespeare - - - - *John A. Paterson* 273

Stoneyhurst College Observatory - - - - *I. J. Kavanagh* 291

The Star Image in Spectrographic Work - - - *J. S. Plaskett* 297

Meetings of the Society at Toronto, 323; Astronomical Notes, 326; Recent Seismographic Records, 328; Summary of Weather of Canada for September and October, 329.

VI. NOVEMBER-DECEMBER.

Notes on Double Stars - - - - - *T. E. Espin* 333

Transmutation of the Elements and the Interior of the Earth *A. Kirschmann* 337

The Theory of Least Squares - - - - *W. F. King* 347

Prelimiuary Orbit of θ *Aquilæ* - - - - *W. E. Harper* 357

Hermann Karl Vogel - - - - - *C. A. C* 360

Meetings of the Society at Toronto, 362; at Ottawa, 364; at Peterborough, 368; Notes from the Dominion Observatory, 370; Astronomical Notes, 376; Summary of Weather of Canada for October and November, 378; Index, 382.

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VOL. I. NOVEMBER-DECEMBER, 1907. No. 6

Notes on Double Stars	*T. E. Espin*	333
Transmutation of the Elements and the Interior of the Earth	*A. Kirschmann*	337
The Theory of Least Squares	*W. F. King*	347
Prelimiuary Orbit of θ *Aquilæ*	*W. E. Harper*	357
Hermann Karl Vogel	*C. A. C*	360
Meetings of the Society at Toronto		362
" " " " " Ottawa		364
" " " " " Peterborough		368
Notes from the Dominion Observatory		370
Astronomical Notes		376
Summary of Weather of Canada for October and November		378
Index		382

Editorial communications should be addressed to C. A. Chant, University of Toronto, Toronto, Ont.

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NOTES ON DOUBLE STARS.

An Apology—Forming a MS. Catalogue—Lines of Work—The Eyepieces, and the Micrometer—New pairs near β Cygni—Σ 1321 and distant comes.

BY T. E. ESPIN.

FIRST of all I must apologize to Professor Aitken for having given his star No. 553 as a new one. The mistake arose in my MS. Catalogue, where in reducing the star's place to the Epoch of the Catalogue, the R. A. was entered ten minutes too small. The mistake was not detected till the arrival of Professor Burnham's Catalogue, and meanwhile the star was given as a new one in my last list of double stars. A further misfortune occurred in the place for 1900, as given by me.

Perhaps a few words about the methods of work may be of interest to my brother amateurs. Till the arrival of Professor Burnham's Catalogue, before any part of the heavens was examined, it was necessary to form an MS. Catalogue of known double stars. The last so drawn up was for the zones 25° to 30° N. Declination. This alone contains nearly 1000 pairs collected from all sides. The places are reduced to 1855·0, and entered in Argelander's Atlas by estimation. At night it is a great convenience and saving of time to be able to avoid the trouble of reading

circles. The work engaged in is twofold, the measurement of neglected double stars, and the search for new pairs. During 1907 little has been done at the former, till it became clear from Professor Burnham's Catalogue which stars needed measurement. Shortly after the Catalogue came to hand letters were received on the same day from Professor Lau of the Urania Observatory, Copenhagen, and from Professor Van Biesbroeck of the Royal Belgian Observatory, suggesting the measurement of all neglected stars between $+0°$ and $+90°$, and the former has undertaken from $+0°$ to $+25°$, and the latter from $+50°$ to $+90°$, while from $+25°$ to $+50°$ is left to me. These pairs, mostly of Herschel, together with possibly all the new pairs found with the 17¼ in., are probably merely optical or with common proper motion, or else of the 61 *Cygni* type. Of the last type only 38 are now known, though cases of common proper motion are more common. Whether both of the classes will turn out to be binaries or a separate species altogether is one of the problems of the future, which our measurements may be of use to settle.

Now as regards actual work at the telescope. First let me call attention to the fact that with the reflector, the positive or Ramsden eyepiece to my eye always gives sharper images than the negative, and I have entirely discarded the use of the latter. The eyepiece in use for the examination of stars is of peculiar construction. It consists of an adapter carrying a Barlow lens and a draw tube, and into the draw tube the positive eyepieces slide. The powers may be varied at pleasure by moving the draw tube in or out. Thus any range of magnification can be obtained. The micrometer is one of some interest. It originally was made by Browning for Lord Lindsay, and passed through the hands of Dr. Copeland into those of Mr. I. W. Ward of Belfast, and last year into mine. The box that contains the movable spider lines is separated from the position circle and supported by three brass pillars, the space between the bore and the circle is filled by a tube of stiff linen. When this is illuminated by a lamp, bright lines are obtained on a dark field. I have not seen this method described anywhere before but believe it is due to

Dr. Copeland. When the micrometer passed into my hands, it became necessary to contrive some method by which the light should fall on the linen, without falling on the eye. I devised the following arrangement. The micrometer was fixed to a large circular piece of strong cardboard. In this was cut a small hole. Below was mounted a concave mirror, and above a plane mirror, at an angle of 45°. The latter is enclosed, so that no light from it reaches the eye, but yet can fall on the linen tube. A beam of light falling on the lower mirror is reflected to the upper mirror, and from thence to the linen tube, while the cardboard disk entirely excludes the source of light from the eye. The illumination is obtained from an electric hand lamp supplied by the Portable Electric Light Co. This is mounted in the following manner. A stout piece of wood was taken and a hole cut in it so that it slips over the tube in which is the rack-work for focusing, at the top and bottom two magnalium rods are carried out, and securely fastened into a light piece of wood. A small clamp was devised to slide on each pair, the lamp is placed between the clamps and screwed up. A weight attached to the opposite side of the piece of wood of the focusing tube, balances the lamp. The whole will then turn round and remain in any position, while a clamp fixes it where desired. The lamp itself will also turn between its clamps or move farther or nearer. The result is that any strength of illumination may be obtained, while the micrometer can be inserted or withdrawn without interfering with the lamp. A piece of red gelatine gives the requisite red illumination when desired. The micrometer is fitted with a Barlow lens of exquisite definition made by Cooke & Co. of York, and so placed that one revolution of the screw equals 5″·00.

In the autumn three little pairs and a wide triple were noted near *β Cygni*. The first is *B. D.* +27°·3419 is 49 sec. *f* and 11′ N. My measures are :—

P 342°·4 *D* 2″·37 Mags. 9·2 10·5. 2nt 07·655.

The second is $3^m\ 6^s$ *f* 35′, S. and is *B. D.* +27°·3431 and the measures

P 336°·4 *D* 3″·87 Mags. 9·1 11·5. 2nt 07·708.

The third, $3^m\ 12_s$ *f* 29′, S. and is *B. D.* +27°·3434, it is a neat little pair in field with the last. The measures are :—

P 50°·5 *D* 2″·12 Mags. 9·1 9·2. 2nt 07·708.

The fourth is a rather wide object $4^m\ 41^s$ *f* 4′ S., and is the north star of two. It is *B. D.* +27°·3438, and the measures are :—

AB *P* 243°·2 *D* 15″·40 Mags. 8·2 10·8 3nt 07·714

AC 205°·5 20″·32 " 11·2 3nt 07·714

As far as I am aware these are all new.

Σ 1321. This is a double star of the 61 *Cygni* type. Some years ago I called attention to the existence of a small star in the field, but it was too faint to measure. This year I have succeeded in measuring it :—

AB *P* 66°·5 *D* 18″·82 2nt 07·202

AC 282°·7 28″·44 *C* = 14·0 2nt 07·202.

The place for 1900, R. A. $IX^h\ 7^m\ 4^s$. Decl. +53° 25′.

Tow Law,

Co. Durham,

England,

October 28th.

TRANSMUTATION OF THE ELEMENTS, AND THE INTERIOR OF THE EARTH.

By August Kirschmann.

OUR knowledge of the planet on the surface of which we live and move is, in proportion to the size of that body, not more than skin deep. We have a fairly good idea of its average specific gravity, and consequently the proof of a necessary increase of density from the surface downwards; but our ideas of the quantitative relations of this increase of density, at least beyond the depth which we have reached in our attempts to puncture the earth's skin, are pretty vague. I think even the question which once puzzled Kant, whether the greatest density is at the centre or at some point between centre and periphery is by no means settled. But it is not this question which shall occupy us in this paper; it is the problem of the physical state and chemical nature of the substances composing that portion of the body of our planet which is out of the reach of our senses and scientific instruments.

We can scarcely be supposed within the compass of this short paper to attempt an exhaustive treatment of the subject. What we intend to do, is merely to emphasize a certain problem which has so far not found the consideration due to it,

That the interior of the earth is in a state of great heat is proved by the increase of temperature in the lower strata of the earth's crust. That the interior of the earth is of a considerably greater density than that of the crust follows from our knowledge of the specific gravity of the whole globe, which is between twice and thrice as great as that of the rocks on the surface, and consequently corresponds to that of some of the heavier metals; and, further, from the fact that the plutonic and volcanic rocks are the heavier the greater the depth to which we have to trace their origin. If the specific gravity of the rocks composing the crust

is between two and three and the average specific gravity of the entire earth is between five and six, then we must assume that the interior must be very dense, perhaps denser than the common metals, iron, copper, etc., and perhaps even heavier than the heaviest metals known to us, as gold and platinum, iridium and osmium. On the other hand, the steady increase of temperature which we everywhere meet when we try to penetrate into the depth of the crust, and which we cannot assume to stop at the limits of our penetration, will necessitate the assumption that the interior is in such a state of high temperature that the substances composing it can neither remain in the solid nor the liquid state, but must be gaseous. Most of them are probably above the critical temperature. I know very well that a certain school of modern scientists claim—and, indeed, in the face of volcanic eruptions and earthquakes which affect more than half of the globe—that the earth behaves like a solid body. They draw this conclusion from certain phenomena, though they have never ascertained whether they could not occur in the very same way in gases under high pressure. We have practically no knowledge of how a gaseous mass in the state of extreme condensation and above the critical temperature would behave. For the conditions in such a mass we have no physical analogy in our ordinary and scientific experience, just as we have on our earth no analogue to such events as seem to occur in the photosphere and reversing layer of the Sun. We even do not know whether or not our ordinary ideas of the chemical elements can be applied to "matter" in this condition. Nobody in our day believes in the absolutely elementary character of the seventy-nine or eighty chemical elements. They are elements only as long as we cannot further analyze them. The fact that they form a regularly graded series with regard to their electrolytic qualities and their atomic weights, their being arranged in groups in which the discovery of the missing links can be predicted with the same mathematical certainty, with which the existence of the planet Neptune was anticipated before its actual discovery, and last, not least, their radio-active properties plainly suggest that they are

either modifications of one or of a few ultimate elements.

They are elements to us only under the prevailing conditions of heat and pressure; they may not be such if these conditions are essentially changed. Withdraw, for instance, all attraction of gravity by bringing an atom or an aggregation of atoms, *i. e.*, a particle, to a sufficient distance from all gravitating influences, and the atoms may disintegrate completely, and the smaller unities of which they are composed, the ions, or electrons, or sub-ions, or the little centres of energy (we could just as well call them the elementary physical and psychical entities), are set free. They form no longer a solid nor a llquid nor a gas, and they cannot be said to correspond to any of the chemical elements. In fact, the properties which characterize a certain chemical element may be the result of the manner in which these ultimate ions or sub-ions or sub-sub-ions coöperate or interact in making up the atom. The customary idea that the separation could only be accomplished by enormous heat is certainly wrong. It can just as well be accomplished by the absence of gravity. The temperature which a substance may have in order to keep up the gaseous state depends on the prevailing conditions of gravity (mutual attraction or pressure). We do not need to assume that the cosmic nebulæ, though they may be true gases, have a high temperature. That they are nevertheless gases is due to the fact that they are still under the influence of some gravity or pressure. If this influence of gravity or pressure is withdrawn or reaches a certain minimum there is nothing left to keep the atoms together, they will dissolve into the parts which constitute them, say, the ions, etc. And this is where nebular and planetesimal hypotheses ultimately must meet and amount to the same thing. "Matter" in this condition will be much less dense than a gas, and yet will have no temperature at all, or at least a very low one. The same conditions which prevail at *low* temperature at the *absence of pressure* can also be produced at *high* temperatures by *excessive pressure*, or, in the case of very great pressure, by *application of extreme heat.* In other words the very same condition which is characteristic of "matter" in the more or less complete absence

of pressure and heat may be brought about by the combined efforts of these two agencies.

According to this view the interior of the earth below the solid crust, the thickness of which we do not know, and whose innermost strata probably continue in a stage of structural instability, must be in the state of an extremely condensed gas (the pressure being furnished by gravity), in temperature far above the critical point, so that the physical molecule and the chemical atom scarcely can exist. They are disintegrated into free ions, for which there is no distinction of physical states of aggregation, and probably no differentiation of chemical elements. Thus, if we are asked which of the known chemical elements constitute the gaseous or hypergaseous interior of the earth, we have to answer: "None at all," for this distinction will scarcely exist there.

Just here I may call attention to the fact that there are phenomena nearer at hand than the interior of the earth, facing which we have to confess that we do not know what is going on. We do not know what happens at the surface of a white hot or incandescent body. We measure the temperature in the neighborhood but not actually at the incandescent surface. Our calculations with regard to intensity and amplitude of the waves of light may be comparatively reliable at some distance from the source of light, but if consistently carried through, they lead to absurdities when applied to the origin of light. Finally, our spectral-analytical discussions are not free from a certain vague fallacy which I wish to point out. If in spectrometrical examinations the appearance of a certain bright double line near $589\mu\mu$ is a proof of the presence of the element of sodium in the burning body or flame, if certain other lines are a criterion for the existence of hydrogen, other systems of lines for iron, calcium, etc., then we should logically conclude that the appearance of *all possible lines together*, which is the same as a continuous spectrum, would indicate the existence of all the elements in the flame or in the incandescent body. But we do not say so; we say a continuous spectrum comes from any substance when in the state of

white heat. This certainly is not very consistent. Logically we should say: We do not know the nature of the events going on at the surface of an incandescent body; the light coming from such a body indicates the presence of *all elements*. We have to assume either that in that part of "matter," where the light forming a continuous spectrum is originated, *all differences of elements are abolished*, or that all elements, known and unknown, are present. And it must not be forgotten that there all transitions from the line spectrum of a gas to the continuous spectrum of an incandescent body. If the gas is under the influence of pressure, the lines are transformed into bands which become the wider the greater the pressure is; and there is no reason to doubt that with sufficiently increased pressure they would blend into a continuous spectrum.

Returning to our problem we claim that the interior of the earth is in the above described hypergaseous state, in which all molecular and atomic structure is dissolved; *i. e.*, the so called chemical elements, which are nothing but modifications of the combination or modes of coöperation and interaction of a smaller number (perhaps two) of ultimate elements, are decomposed into their constituents. As what element this gaseous mass appears on the surface depends on the conditions of temperature and pressure prevailing at the time at that place where the mass was permitted to leave that hypergaseous state; in other words it depends upon the pressure existing where this gaseous mass is allowed to cool off below its critical temperature.

Thus, while in the interior of this great gaseous ball there are no longer any elements, there must be near its periphery, *i. e.*, below the hardened crust, a zone of instability where masses leave and reënter that state, and where consequently, on account of the changes in pressure, *transmutation of elements* may often take place. What happens in this case may be best seen from a somewhat imaginary experiment, which I propose simply to illustrate the logical correctness of the above deduction, though I shall by no means admit that the experiment, fictitious as it may be, is inexecutable. So little, indeed, did I doubt the feasibility of this

crucial experiment when the idea first occurred to me, that seven or eight years ago I communicated the idea through my friend Professor Lang to Sir William Ramsay. Suppose you have an element, say silver or hydrogen, in gaseous form; give it a very high temperature, such a temperature that it is above the critical point, *i. e.*, beyond that temperature above which the gas can by no pressure be forced to assume the liquid or solid state. Then apply sufficient pressure and condense the gas until it has a volume smaller than it would have in its liquid or solid state. Then let it cool off without reducing the pressure. Here the question arises: If the temperature now falls below the critical point what will become of our element? It can no longer be silver or hydrogen, for its volume, and consequently its specific gravity, its density are changed. It must be something else. It must now be a substance of a greater density and smaller volume, *i. e.*, another element. *It is transmuted.*

Of course I do not claim that it would be advisable to start the experimental investigation of this subject by trying to make gold out of silver or sodium out of hydrogen. At first we had better start with some "easier" element whose freezing point and critical temperature are a little more easily within our reach. And even in this case I realize that we will meet with considerable difficulties in producing the requisite heat and pressure. But there is nothing to preclude the theoretical possibility of the experiment, unless the first principles of modern Physics, the mechanical theory of heat and the kinetic-gas-theory are abandoned. For, the only objections which could be raised would be:

1. That the gaseous element under the aforementioned conditions of a condensation greater than that in its liquid or solid state *would refuse to cool off.*

2. That the material, though being a gas and though there can be no limit to the possibility with regard to the quantity of pressure, *would refuse to be condensed.*

Both these objections are, according to our modern theories of entropy and conservation of energy, perfectly absurd.

From the standpoint of the above considerations certain well-known but unexplained facts will appear in a new light. Is it not curious that the metallic elements belonging chemically to the same group are so often found together? There is scarcely any native gold found on this globe which does not contain silver (up to 40 per cent.) and sometimes palladium (up to 10 per cent.) The metals of the so-called platinum group comprising platinum, rhodium, rhutenium, osmium, iridium, and palladium, are always associated in the ores. With lead traces of silver are always found. Iron, cobalt, nickel, molybdenum, and manganese accompany one another in the ores and even in extra-telluric meteoric masses. This mystery is solved if we assume that they all have their origin from the same gaseous or hyper-gaseous magma, that one portion had to become iron, another cobalt, another nickel, according to the conditions of pressure under which they had to cool; that one particle had to go into surface existence as gold and its neighbor as silver, simply because they cooled off below their critical temperature under different conditions of pressure. Only thus can we understand why these elements, so closely related to one another by their chemical characteristics, are produced by nature in such intimate alloys.

We may conclude with a few philosophical observations. We have put the term "matter" always in quotation marks because we regard matter only as an auxiliary conception which from the standpoint of the economy of thought has been quite useful in the past, but which, so to say, is no longer needed. Since noted chemists attempt to build up a view of the world without matter, and since even that most fundamental function of matter, "Mass," which was so long thought of as being absolutely unalterable, lately becomes somewhat shaky, the conception of "matter" can be nothing more than a stop-gap like the expressions "thing," "something," or "it," and so on.

Another remark seems to us to be necessary, mainly with regard to the term "element." Can the elements at which physical and chemical analysis arrive ever be final, and yet be

regarded as something objective, something independent of the conceiving mind? I think not. We either assume that the elements quantitatively, *i. e.*, physically (atoms, ions, etc.) as well as qualitatively, are independent of the perceiving mind, and then there will be no limit to their further division and analysis, or we must admit that they are structures in consciousness, and then they must be, like all other states and complexes of states of consciousness, subject to psychological analysis. Since we can know of nothing unless it has a part in our consciousness, *final* elements must always be as states of consciousness. Atoms, ions, forces, energies, can never figure as last elements in "knowledge." They must remain as such, "matters of belief." If we wish to claim certainty (knowledge) for our propositions of physics and chemistry, we must be able to define all expressions used in terms of ultimate psychical elements, *i. e.*, elementary states of consciousness, which themselves, of course, defy all attempts at definitition, description and explanation, and which consequently must be taken as they are, whether you call them nature or miracles. They alone, and that which can be deduced from them by making use of their actual and possible relations, can be called "facts," and every scientific conception, distinction, and proposition which cannot without contradiction be defined in terms of these ultimate psychical elements must, if claimed as a certainty, be a deception—a sham knowledge.

Which, then, are the ultimate elements of all reality, in so far as it concerns our "knowledge" of the external world? They are the sense qualities and their axiomatic mathematico-logical relations. And all the conservation of quantities as conservation of energy, conservation of the quantity of matter, etc., will, when reduced to facts or knowledge, *i. e.*, when stripped of all dogmatic presuppositions, and unfounded though fashionable and age-long cherished assumptions, reveal themselves as nothing else than the conservation of the visual and tactual field of space.

The sense qualities and their geometrical relations in space are the real elements of which the external world, in so far as it

concerns the given facts, is made up, and a thing or event is "known" when it can be definitely and without contradiction represented and stated in terms of these elements. But there exists a certain law, *i. e.*, one of the fundamental mathematico-logical axioms, which regulates the number of possible elements in a manifoldness. This law, which I have elsewhere discussed and I think demonstrated,* demands that there are with regard to a manifoldness of elementary qualities, presupposing they are not absolutely disparate, only three possibilities; namely, there is —

either *no differentiation*, *i. e.*, one element, or, what is the same thing, no element; or

there are two antagonistic elements; or, finally,

there is an infinite number of elements.

There is no further case possible. All qualitative manifoldnesses have to obey this law. Thus, for instance, the systems of light and color qualities do so. A being endowed with vision has either a one-dimensional system of light sensations without color distinctions (total color blind) or a bi-polar system of two antagonistic qualities (dichromatic), or he sees many, *i. e.*, an unlimited number of qualities (polychromatic). In the same way the qualities with regard to space-direction—the differentiation of directions in space is qualitative, though it admits of quantitative consideration—have to obey this law; consequently there are in a spacial system,

either no directions, (a mathematical point),

or two antagonistic directions, (a line),

or, finally, an infinite number of directions, (a plane or solid).†

If chemical analysis aims at the discovery of really ultimate elements, it must admit also for these the validity of the above stated law. In other words, if the world is to be considered as

* "Normale und anomale Farbensysteme," *Archiv für die gesamte Psychologie*, Vol. VI, page 404 ff.

† The three fundamental directions or dimensions of space are conventional. See my publication *Die Dimensionen des Raumes* (Leipz'g, with Engelmann, 1902).

consisting of chemically ultimate elements, the number of these elements can only be *one*, *two*, or *infinite*.

That these statements sound at first so paradoxical is the result of a many thousand years old habit of starting our scientific enquiries and researches at the wrong end, viz., with certain preconceived ideas, instead of with the given facts. Thus, for instance, physics starts out with the conceptions of mass, energy, etc., which are not facts at all, but only—and, indeed, very useful—auxiliary conceptions, unless they are stated in terms of ultimate elements. All facts are states of consciousness, and so long as the exact sciences do not condescend to express their conceptions, distinctions, and propositions, in terms of given elementary facts, and necessary mathematical relations, they cannot hope to arrive at a consistent view of the world.

THE THEORY OF LEAST SQUARES.

By W. F. King

DISCORDANCE between observations tells us that errors exist. Though the greatest attainable accuracy has been used, differences still are found.

From the discordant results to find the best value of the quantity sought, and to test the accuracy of the observations by some standard of accuracy, is the object of the method of least squares.

We distinguish different classes of errors. The principal are:

Constant errors which occur always with the same sign and of the same magnitude, affecting all the observations of a series alike.

Periodic errors which may take any value between certain limits, but recur at regular intervals of space or time.

Accidental errors which may also take any value between certain limits, but do not recur at regular intervals; they exhibit the irregularity of arrangement called "random."

An error may partake of the character of more than one of these classes. For example, in the observation of occultations of stars by the Moon, the instant of emersion is likely to be observed too late, but not by the same amount in all cases. This is an example of a constant error combined with an accidental one. The periodic error of a micrometer screw, again, may have an accidental element, due, say, to looseness of the screw in its nut.

The constant and the periodic errors, or parts of errors, have to be eliminated by the use of proper means. The remaining accidental errors alone are the subject of the theory of least squares.

It follows from the above that it is characteristic of accidental errors that, in a long series of observations, positive and negative errors occur equally often. For, if there were a tendency one way rather than the other, a part of the error would, by definition, be called constant error.

Again, we may distinguish between errors, by reference to magnitude. We may have large errors, that is, errors of magnitudes measurable directly with our instruments. By varying the method of observation (*e. g.*, by reversal, etc.) the amount of the error may often be ascertained, and thus eliminated. The periodicity of an error may indicate a physical cause, whose effects may be calculated by theory, or which may be obviated by improvement in methods.

When all such errors have been got rid of we have left the accidental errors. However observations may be improved, experience shows us that there is always a residuum. We must attribute this either to a varying cause or to a number of causes operating together.

As we can always recognize the existence of many disturbing causes, such as (in astronomical observations) irregularities in refraction, tremors of the instrument produced by wind, expansions or contractions of the parts of the instrument due to changes of temperature, and many others, it is natural to adopt the hypothesis that the accidental errors are the resultant of a great number of elementary errors, each of which must be small, because the aggregate of them is small. The parts of the effect due to the separate causes we cannot determine on account of the complexity in which they are mingled together. It is necessary for us to deal with them by statistical methods.

Since the resultant errors, *i. e.*, the errors of observation, are by supposition as often negative as positive, it easily follows that the elementary errors are likewise.

Hence, in the long run, the positive and the negative resultant errors are of equal amount. From this it follows that the arithmetic mean of a series of direct observations is the best value for the quantity sought.

This is on the hypothesis that the errors are the sums or resultants of numerous elementary independent errors.

On the other hypothesis, that the accidental errors are due to one varying cause (or to a limited number of varying causes), the principle of the arithmetic mean also follows.

Thus, let us have two observed values, a and b, of a measured length for example, which we consider equally good. As the best value for these observations we must assign the mean $\frac{a+b}{2}$, since that is equally distant from a and b. To assign any other value, which would be nearer to a or to b than this, would evidently amount to giving more value to one of the two observations than to the other, which is contrary to hypothesis.

If we have three observations, a, b, c, all of equal value, we may, by taking them two and two, get from b and c the best value $\frac{b+c}{2}$; from c and a, $\frac{c+a}{2}$; and from a and b, $\frac{a+b}{2}$. We have thus three means (which we may denote by a_1, b_1, c_1) which are evidently equal in value. It will be seen also that the range between the greatest and the least of a_1, b_1, c_1 is less than that between the greatest and the least of a, b, c. Combining a_1, b_1, c_1 in the same way, two and two, we get a_2, b_2, c_2, which have a smaller range again than a_1, b_1, c_1. Proceeding in this way, the range continually diminishing, becomes zero in the limit, the three values coincide, and give the one value $\frac{a+b+c}{3}$ or the arithmetic mean as the best value for the quantity sought.

If, now, we have four quantities, we can combine them three and three in four ways, and thus get (by the preceding proof) the best value from each of the four groups of three observations. Combining the four best values again, three and three, we get new values, and continuing we shall as before arrive, in the limit, at the arithmetic mean.

Finally, we may prove that if the rule of the arithmetic mean holds for n quantities, it will hold for $n+1$. Whence, since it holds for 3, it holds universally.

The validity of this proof that the arithmetic mean is the best (or "most plausible") value of the quantity sought depends upon the assumption that the errors are, in magnitude and direction, independent of the magnitude of the quantity measured.

If the case were otherwise some other function than the arithmetic mean would have to be used.

We have then the postulates that accidental errors of given amounts occur in the long run with equal frequency, positive and negative, and that their amounts are independent of the magnitude of the quantity sought.

We shall now consider a few of the proofs which have been given for the law of error, so as to show what further assumptions are made in them.

FIRST PROOF OF LAW OF ERRORS.

The first proof of the law of error which I shall consider is that which is most usually given in the text books. It is as follows:

It is first shown that, if a number of direct measurements of a quantity are made, all the observations being equally good, the arithmetic mean is the most "plausible" value of the quantity, or the best value to take, considering the evidence at hand.

Then, assuming the arithmetic mean, as the most plausible value, to be also the most probable value, it is desired to find the "law of error," or a mathematical expression for the relative frequencies of different errors.

Appeal is made to experience to show that in a series of observations, the various results are grouped about a central value in such a way as to suggest that small errors are more frequent than large ones. Hence it is assumed that the probability of an error is a function of its magnitude. Hence the probability of the occurrence of an error between v and $v+\delta v$ may be represented by $\varphi(v)\delta v$, and in any series of observations the probability of the simultaneous occurrence of errors $v_1, v_2, \ldots v_n$ will be proportional to the product $\varphi(v_1)\varphi(v_2)\varphi(v_3)\ldots\varphi(v_n)$.

It is now required to find the form of the function so that this expression shall be a maximum, when $v_1, v_2 \ldots v_n$ are the residuals from the arithmetic mean.

Hence we have $\varphi(v_1)\varphi(v_2)\ldots\varphi(v_n)$ a maximum with $v_1+v_2+\ldots+v_n=0$.

By the application of the infinitesimal calculus we find

$$\varphi(v) = ce^{-h^2v^2}$$

The constant c is determined from the consideration that the sum of the probabilities of all possible errors shall be equal to unity. That is

$$\int_{-a}^{a} ce^{-h^2v^2}dv$$

is to be put equal to unity, where a is the magnitude of the greatest possible error. But as this greatest error cannot be determined, the limits of integration are extended to plus and minus infinity.

Then since

$$\int_{-\infty}^{\infty} e^{-h^2v^2}dv = \frac{\sqrt{\pi}}{h},$$

$$c = \frac{h}{\sqrt{\pi}}$$

and the probability of an error between v and $v + \delta v$ is

$$\frac{h}{\sqrt{\pi}}\, e^{-h^2v^2}\, dv$$

If we draw the curve

$$y = \frac{h}{\sqrt{\pi}}\, e^{-h^2x^2}$$

we get the so-called probability curve.

Substituting this value of $\varphi(v)$ in the maximum equation we get

$$\varphi(v_1)\,\varphi(v_2) \ldots\ldots \varphi(v_n) = \frac{h}{\sqrt{\pi}} e^{-h^2(v_1^2 + v_2^2 + \ldots + v_n^2)}$$

which is the maximum when $v_1^2 + v_2^2 + \ldots . v_n^2$, that is, the sum of the squares of the residuals is a minimum. This is the principle of "least squares."

REMARKS UPON THE FIRST PROOF.

In this proof the following assumptions are made.

1. That the most plausible value is the same as the most

probable value, or that the probable value from the observations is identical with the true value.

The central value about which, as it is said, the observations are found to be grouped must be the true value of the quantity measured, and hence to take the residuals, v, from another quantity, namely, a value derived from a limited number of observations, is not quite logical. It is true that under our postulate of equal positive and negative errors of equal freqnency in the long run, the mean will coincide with the true value, but only when the number of observations is infinitely great. The procedure may be justified as an approximation merely, since the arithmetic mean approaches the true value as the number of observations increases.

Reference may be had to the works of Bertrand and Poincaré on the theory of errors for the discussion of the distinction between the "probable" and the "most probable" value.

2. We have already spoken of the assumption that the probability of an error is independent of the magnitude measured. This seems a reasonable assumption in the measurements of astronomy and geodesy.

3. The proof under consideration further assumes that the probability of an error is a function (of fixed form) of the magnitude of an error, and that this function is a continuous function (for it is differentiated).

This assumption of continuity is equivalent to assuming that errors of all magnitudes are possible, so that the error of an observation is a variable quantity capable of taking any value between zero and some indefinite limit. Nothing is said, as the proof is ordinarily given, of any physical reason for this property.

A plausible reason may be found in the assumption that the error is the resultant of a great number of elementary errors. This assumption is the basis of some of the proofs of the law of error which follow, and which seem, on this point, to be superior to this first proof.

That the probability should be a determinate function of the

magnitude of the error also does not seem to be altogether reasonable except on the hypothesis of elementary errors.

4. Another assumption is that, in the determination of the constant c, the limits of integration may be extended from a, which represents the greatest error possible in the circumstances of the observation, to infinity. Theoretically, this extension makes the constant c or $\frac{h}{\sqrt{\pi}}$, smaller than it should be. As will be seen later, however, the area of the curve of probability beyond a certain point is very small in comparison with the rest, so that the error is practically insignificant.

This is nevertheless hardly evident, *a priori*. I imagine most students, in their first reading of the subject, look upon the extension of the limits as a convenient device for simplifying the integration.

5. It is pointed out by Poincaré that the product

$$\varphi(v_1)\ \varphi(v_2)\ \ldots\ \varphi(v_n)\ dv_1\ dv_2\ \ldots\ dv_n,$$

representing the probability of the simultaneous occurrence of the system of errors v_1, v_2, v_n, should contain a factor $\psi(z)dz$, expressing the probability that the most probable value z, from which the v's are measured, should lie between z and $z + dz$.

Although not evident *a priori*, it may be proved that $\psi(z)$ is a constant. Its omission therefore does not affect the result.

SECOND PROOF.

This proof is similar to the first, but somewhat different in form.

If $x_1, x_2 \ldots x_n$ are the measurements, and z any assumed value of the quantity measured we have the residuals

$$v_1 = z - x_1$$
$$v_2 = z - x_2$$
$$\ldots\ldots$$
$$v_n = z - x_n.$$

Squaring and adding, we have

$$[v^2] = [x^2] - 2z[x] + nz^2$$

$$= n\left\{\frac{[x]}{n} - z\right\}^2 + [x^2] - \frac{[x]^2}{n}$$

Here z is the only variable.

The least value of $[v^2]$ is when $z = \frac{[x]}{n}$ the arithmetic mean of the measurements.

Therefore, for the arithmetic mean, the sum of the squares of the residuals is the least possible. This is the property from which the "method of least squares" takes its name.

Observe that the principle of the minimum sum of squares does not appertain to observations as such; it is the property of the arithmetic mean.

The principle owes its applicability to errors of observations to the assumption that the arithmetic mean of the measurements is the most probable value of the quantity measured.

Adopting now the arithmetic mean as the most probable value, we have to find the form of the function φ, so that

$$\varphi(v_1)\ \varphi(v_2)\ \ldots\ \varphi(v_n)$$

may be a maximum when

$$v_1^2 + v_2^2 + \ldots + v_n^2$$

is a minimum.

Assuming as before that φ is a continuous function, take the logarithm of the maximum function.

The logarithm of $\varphi(v)$ will be a function of v; call it $F(v)$.

Then, $F(v_1) + F(v_2 + \ldots + F(v_n)$ is a maximum and $v_1^2 + v_2^2 + \ldots + v_n^2$ is a minimum.

It is evident that the form of F must be $a - h^2v^2$, (the coefficient of v^2 being essentially negative), whence

$$\varphi(v) = e^{a - h^2v^2} \text{ or } ce^{-h^2v^2} \text{ as before.}$$

THIRD PROOF.

This proof is given as an alternative to the first proof in Wright and Hayford's *Adjustment of Observations.*

It has been pointed out that in the first proof it is tacitly assumed that the error of an observation may take any value + or − from zero to the limit set by the conditions of the obser-

vation. Thus infinitesimal changes in the errors are admitted to be possible.

The present proof assumes as a physical cause for this, that each error is made up of an infinite number of infinitesimally small elementary errors. These elementary errors are supposed to be equal in magnitude, and *all* to enter into every observation, but indifferently with the plus or minus sign.

Let the elementary errors number $2n$, and call the magnitude of each ϵ.

If $n+m$ are plus, and $n-m$ minus, the error $2m\epsilon$ results. This can happen in $\frac{2n(2n-1)(2n-2)\ \ldots\ (n+m+1)}{1.2.3\ \ldots\ (n-m)}$ ways. Or in general, the number of ways in which an error $2m\epsilon$ may occur is the efficient of x^{n-m} in the expansion of the binomial $(1+x)^{2n}$.

Calling 2ϵ, dv, and putting f for the frequency of the error mdv, we have

$$f = \frac{2n(2n-1)\ \ldots\ (n+m+1)}{1.2\ \ldots\ (n-m)}\ .$$

For the consecutive error $(m+1)dv$, which we may call $v+dv$, we have

$$f+df = \frac{2n(2n-1)\ \ldots\ (n+m+2)}{1.2\ \ldots\ (n-m-1}\ ,$$

whence $\frac{df}{f} = -\frac{2m+1}{n+m+1} = -\frac{2v+dv}{ndv+v+dv}$.

Now, since dv is an infinitesimal, and infinitely small compared with v, we have

$$\frac{df}{f} = -\frac{2v}{nd+v}.$$

Again, since df is indefinitely small compared with f, and v is indefinitely small compared with $ndv+v$, whence v in the denomination may be neglected, and we have

$$\frac{df}{f} = -\frac{2v}{ndv}.$$

Since v is indefinitely small compared with ndv, but indefinitely great in comparison with dv, n must be of an order of magnitude comparable with $\frac{1}{(dv)^2}$, and $n(dv)^2$ must be a finite

constant. Calling this constant $\frac{1}{h^2}$, we have

$$\frac{df}{f} = -2h^2 v dv.$$

Integrating and calling the value of f, when $v = 0$, f_0

$$f = f_0 e^{-h^2 v^2}$$

The probabilities being proportional to the frequencies

$$\varphi(v) = ce^{-h^2 v^2} \text{ as before.}$$

The omission of dv in the numerator of

$$\frac{df}{f} = -\frac{2v + dv}{ndv + v + dv}$$

may be criticized, for when m is small dv may not be small compared with $v(mdv)$. In fact the error $mdv = 0$ has a greater probability, or occurs more frequently than any other specified error.

The proof fails to show what error may result from the assumption that the elementary errors are infinitesimal and infinite in number. Although we know that the elementary errors in any observation are numerous, we can hardly assume that they are infinitely numerous.

The following proof will afford some means of estimating the inaccuracy introduced in passing from the finite to the infinite.

(To be Concluded).

PRELIMINARY ORBIT OF θ AQUILÆ.*

By W. E. Harper.

THE star *θ Aquilæ*, $\alpha = 20^h\ 6{\cdot}2^m$, $\delta = 1^\circ\ 7'$, photographic magnitude 3·6, was one of the early-type stars selected for observation when the single-prism spectrograph was ready for use about the middle of May of this year. Owing to lack of help the early spectrograms were not measured until the latter part of August. It was then noted that there was a rapid change at the time of maximum positive velocity and that this condition repeated itself about every 17 days. This bore out Deslandres' † conjecture of a period of $16{\cdot}8^d$ and high eccentricity (0·6). I do not see just how his observations, made some six years ago, can give more than a conjecture as to the period or the eccentricity of the orbit as they are too few in number, and the residuals from the best curve which can be drawn through them are exceptionally large. He makes a suggestion of a secondary period, but our observations show no such indication.

Though we have tried to obtain spectrograms of the star in all its phases, and particularly at the time of periastron, there are still gaps in the oscillation curve where unfavorable weather prevented observations and, pending the securing of these, I have thought it well to give the provisional elements.

The spectrum of the star is of the type VII *a*, the four hydrogen lines H_β, H_γ, H_δ and H_ϵ, along with the magnesium λ 4481 and the K line λ 3933, being well adapted for measurement. The following is a summary of the velocities obtained. Where two or more spectrograms were secured on one night the wighted mean of the velocities is used.

* Communicated by permission of the Director, Dr. W. F. King.

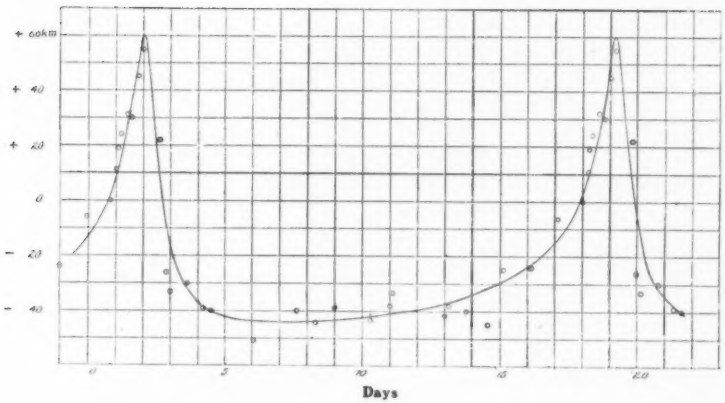
† *Bull. Astr.*, 20, 129, 1903.

SUMMARY OF VELOCITIES.

Date	G.M.T.	Velocity	Date	G.M.T.	Velocity
1907	h m	km.		h m	km.
May 31	19 34	−24.	Oct. 1	12 03	+28.
June 10	18 53	−39.	" 1	12 32	+33.
" 12	20 05	−38.	" 1	13 09	+30.
" 14	19 30	−42.	" 1	14 43	+25.
" 20	18 40	+45.	" 1	15 24	+36.
" 21	19 20	−26.	" 2	12 41	+18.
July 2	18 38	−40.	" 2	13 49	+28.
" 8	18 37	+22.	" 2	14 08	+23.
" 9	17 05	−30.	" 2	14 36	+26.
" 13	16 53	−40.	" 2	14 59	+12.
" 16	17 07	−43.	" 18	12 15	+28.
" 20	18 19	−45.	" 18	13 45	+33.
" 27	18 15	−40.	" 25	12 52	−44.
Aug. 10	15 50	+19.	Nov. 4	10 28	+16.
" 13	16 37	−39.	" 4	10 51	+32.
" 15	13 45	−51.	" 16	12 03	−38.
" 27	15 07	+10.	" 18	10 45	−31.
" 27	16 04	+11.	" 18	12 37	−20.
Sept. 6	14 41	−38.	" 19	10 41	−18.
" 14	16 10	+55.	" 19	11 00	−30.
" 30	13 02	+3.	" 30	11 00	−6.
" 30	13 25	+4.	" 23	12 42	−33.
" 30	14 08	−7.			

The value of P which suited these observations best, and which was finally decided upon was 17·17 days. When all the observations were reduced to one period the resulting curve was treated by the methods of Russell, Lehmann-Filhés and Schwarzschild, and the following elements were adopted as best suiting the observations.

$P = 17{\cdot}17^{d}$
$V_{o} = -26{\cdot}7$ kms. per sec.
$e = 0{\cdot}725$
$\omega = 20^{\circ}$ (measured from ascending node)
$T = 1907$, Oct. 2.15 G.M.T.
$a \sin i = 8{,}455{,}500$ kms.



Computed Velocity Curve of θ *Aquilæ*, with Observed Points.

The curve shown in the figure is computed from these elements, and with the exception of one or two cases the agreement is all that could be expected.

DOMINION ASTRONOMICAL OBSERVATORY,
OTTAWA, DECEMBER, 1907.

HERMANN KARL VOGEL.

HERMANN Karl Vogel was born in Leipzig April 3, 1842, his father being a well-known schoolmaster. After studying at the Polytechnic at Dresden he entered in 1864 the University of Leipzig, making mathematics and astronomy his special studies. In 1865, while still a student, he was appointed assistant at the Leipzig Observatory and remained there until 1870 in which year he was made director of the private observatory of von Bülow at Bothkamp, near Kiel. This position he held for four years, in that time establishing for himself a high position in the world of astronomy. In addition to the *Bothkamp Observations*, he published in 1874 *Researches on the Spectra of the Planets*, which work was "crowned" by the Copenhagen Academy of Science. At Bothkamp was begun, also, work on the determination of radial velocities.

In 1874 Vogel accepted a position in the newly-established Astrophysical Observatory at Potsdam, and eight years later he became its director. The valuable series of *Publications* attest the devotion and skill of the director and his distinguished assistants, among whom may be named Spörer, Scheiner, Müller, Kempf and Hartmann.

With Scheiner's assistance photography was applied to the radial velocity measurements, and a new era was begun in this work. With a 11-inch refractor the spectra of 51 bright stars were photographed, and the results, published in 1892, were at once recognized as of the highest value.

Another interesting and valuable research dealt with the variable star Algol. It had long been conjectured that Algol had a dark companion, that the two bodies revolved about their common centre of gravity and that the earth was almost in the plane of the orbit. When the dark body came between us and the bright one a partial eclipse took place and the minimum of Algol resulted. Now if this explanation be true it is evident

that before the minimum the bright star must be receding from us and after the eclipse it must be approaching us. In 1889 Vogel showed that such was the case, and that from 12 to 18 hours before obscuration Algol is moving away from us at the rate of nearly 27 miles per second, while after minimum it approaches us at the same rate. This was the first discovery of a spectroscopic binary. In 1890, Vogel found that Spica was also a spectroscopic binary, with a period of about four days.

Vogel's classification of stellar spectra was first published in 1874 and revised in 1894. It is based on that of Secchi, but proceeds more definitely upon the idea of development as indicated in the star's spectrum.

A man so eminent in his line of work was naturally chosen member of many scientific societies. In 1892 he became a member of the Berlin Academy of Sciences, in 1895 he received the Prussian Order of Merit and in 1899 he was made a Privy Councillor of Germany. Since January 1904, he has been enrolled as an Honorary Fellow of the Royal Artronomical Society of Canada.

Vogel died at Potsdam on August 14, 1907, and the funeral service was fittingly held in the dome of his great telescope.

C. A. C.

MEETINGS OF THE SOCIETY.

AT TORONTO.

October 29, 1907.—The following were elected members of the Society :

James Young, Magnetic Observatory, Toronto.

Miss Marie C. Strong, Toronto.

Miss L. M. Mason, M.A., Toronto.

Mrs. G. P. Ham, Toronto.

Mrs. E. J. Clarke, Toronto.

Walter Keast, B.A., Toronto.

Bulletins from the Harvard College Observatory, respecting Mellish's Comet were read, and it was stated that the comet had been observed at the Dominion Observatory, Ottawa. Reference was also made to Mr. Mellish, the youthful discoverer of the comet, and attention was directed to an article in *Popular Mechanics*, in which he described how he had made his telescope.

The paper of the evening was by Mr. A. R. Hassard, B.C.L., and dealt with the approaching Transit of Mercury. The paper was prefaced by a vigorous protest against the excessively technical terminology employed by some writers of scientific works. The form and peculiarities of Mercury's orbit were explained and also the manner in which the transits occur. The various appearances of the planet near ingress and egress were described with the assistance of lantern slides. The times and position with respect to the Sun's disc for the coming transit were given in detail, and some of the uses of observations of the transit were mentioned.

November 12.—The following were elected members of the Society :

Miss Susan Gamble, Eglinton, Ont.

Dr. W. M. Wunder, Toronto.

Mr. J. R. Collins referred to a Harvard College Observatory Bulletin relating to certain "knots" seen on Saturn's Ring by

the observers at Lick Oberevatory and confirmed at the Lowell Observatory, and stated that he thought the effect to be probably optical. He had made some experiments to illustrate the effect which he hoped to explain more fully should they prove entirely successful.

Mr. Elvins stated that very large spots were on the Sun at present, the group to which he had drawn attention to a month ago having again come into view.

The President informed the members present that Mr. R. S. Muir had presented to the Society an Addressograph (including oak cabinet) which will be of great assistance in addressing the notices, etc., sent to the members. Hearty thanks were tendered Mr. Muir for his handsome gift.

The paper for the evening was by Professor L. B. Stewart on "Variation of Latitude." A history of early suspicions of variation was given and details of the work of Chandler, Küstner and the International Geodetic Association. The manner of observing for the latitude at the various stations established for that purpose was described, and the extraordinary motion of the earth's pole was illustrated by diagrams. The entire motion is confined within a circle of diameter 60 feet.

November 26.—The following were elected members of the Society :

A. H. D. Ross, M.A., M.F., University af Toronto.

J. S. Porter, Stayner.

R. T. Martin, Toronto.

Reports of meetings of Councils held on the 12th and 22nd instants were read. The latter, in pursuance of the By-Laws, contained the names suggested by the Council of Officers for the year 1908. No further nominations being made the following were declared duly elected officers of the Society for 1908 :

Hon. President.—Dr. W. F. King.

President.—W. B. Musson,

1st Vice President.—A. T. De Lury,

2nd Vice-President.—L. B. Stewart,

Secretary.—J. R. Collins,

Treasurer.—George Ridout,

Recorder.—Miss E. A. Dent,

Curator.—R. S. Duncan,

Councillors.—Joseph Pope, C.M.G., A. F. Miller, Rev. D. B. Marsh, L. H. Graham, Dr. A. D. Watson.

Dr. Chant reported that in company with Messrs. Blake and Clipsham he had observed the Transit of Mercury, on the 14th instant with the 6-inch refractor of the Toronto Observatory, the atmospheric conditions being excellent. The image of the Sun was projected on a screen so that several could observe it at the same time.

Mr. John A. Paterson referred to an interesting article in *World Wide* entitled "Are new Moons being formed?" an hypothesis suggested by the recent formation of "knots" in Saturn's Ring. Mr. Elvins spoke of the danger of mistaking satellites seen against the Ring when at its "thinnest" phase, for knots in the Ring itself.

The paper for the evening was by Mr. John R. Collins on "Modern Theories of the Evolution of Matter."

E. A. D.

AT OTTAWA.

October 17, 8 P. M.—Dr. W. F. King, President of the Section, gave a very interesting paper on the "History of Astronomy." After some preliminary remarks upon the advantages of the historical treatment of any subject he proceeded to trace the history of the development of the science, beginning with the shepherds of Chaldea, who from their nightly watchings doubtless formed some rude notions of the mechanism of the universe. After touching upon early Egyptian and Chinese astronomy he discussed what may be called the second period extending from 500 or 600 B.C. to about 150 A.D. Attempts at precise observations were made during this epoch and some ideas then held were remarkably close to the truth. This period ended with Ptolemy whose theory held almost undisputed sway for fourteen centuries and whose authority prevented progress during that

interval, which Dr. King considered as the third period. The fourth period dated from the sixteenth century. Copernicus with his heliocentric theory of the solar system followed by Kepler's discovery of the true form of the planetary orbits inaugurated this epoch and these were followed by Newton's explanation of the physical cause. The evolution of the modern astronomical instruments from their primitive forerunners was then described, Dr. King speaking briefly on refracting and reflecting telescopes, transit instruments and clocks. The fifth period referred to by Dr. King embraces the application of the spectroscope and photography to astronomical observation, and a short history of the development and progress of astrophysics was given.

Two questions had been handed to the Secretary for the Question Box.

1. Why do not planets twinkle like the stars?

2. How are geographical positions determined astronomically?

These questions were satisfactorily answered by Messrs. Stewart and McDiarmid and added to the interest of the meeting.

November 7, 3 P.M.—Mr. J. S. Plaskett read a paper on "The Star Image in Spectroscopic Work." After a résumé of the conclusions reached in the first paper on this subject given last January. Mr. Plaskett gave an interesting account of the obtaining and mounting of the new corrector and of the tests made. On discovering that only slight improvement over the old lens had been obtained the new corrector was taken to Allegheny for refiguring, and a description of the methods of testing employed was given. The process of figuring the surface to introduce the requisite amount of spherical aberration was then fully described. Tests of the refigured corrector showed that the attempt at improving the performance had been very successful, the image given by the combination of objective and corrector in photographic light being freer from aberration than the visual image given by the objective alone.

A description of the tests employed, by photographing star images, trails and spectra, to determine the effective diameter of the star image, was then given, and the conclusion was reached

that the minimum diameter of the condensed part was about 2″ and that this core was surrounded by a diffuse portion 5″ or 6″ in diameter. Experiments showed that slits of normal width only transmitted about 30 per cent of the incident star light and the importance of designing spectrographs to use as wide slits as possible was pointed out. It was estimated that the new correcting lens shortened the required exposure time by at least 30 per cent.

November 14, 8 P.M.—Dr. King read a paper by Joseph Pope, C.M.G., on "The Dawn of Astronomy." The paper began by recounting some of the advantages and delights of the study of astronomy. It gave a brief description of the appearance of the heavens and explained the apparent motions of the stars and planets. The writer imagined himself placed in the position of an early observer and tried to follow the gradual evolution of the science. He explained the puzzling motions of the various heavenly bodies and described how these motions were traced and followed by the early astronomers. The motion of the Sun amongst the stars, which is not so readily observed as the other motions, must nevertheless have been defined at an early age. The next step developed was the marking of the seasonal changes by the solstices and equinoxes, and the writer proceeded to describe the wonder and fear occasioned by eclipses of the Sun. The gradual discovery of their occurrence after certain intervals was discussed and a graphic account of the feelings of the ancient astronomer who made the first successful prediction of a total eclipse of the Sun was given.

Three questions for the Question Box had been sent in.

1. Why do the Sun and Moon appear larger near the horizon?

2. What connection is there between sun-spots and earthquakes, the weather, etc.?

3. Why do stars near the horizon in the north appear to move from west to east and in the south from east to west?

These questions were satisfactorsly answered by Dr. King, Dr. Klotz and Mr. Motherwell, respectively.

November 29, 3 P.M.—Mr. W. E. Harper, M.A., gave a paper on "The Orbits of Stellar Systems." The paper began by describing the discovery of visual double stars and the further discovery that some of the pairs were in relative motion, From successive measures of the positive angle and distance, the elements of the orbits of some of these binary systems had been determined. He then went on to speak of spectroscopic binaries in which the velocity of one, or, rarely, of both components can be measured by the aid of the spectroscope. The principles of the spectroscope were briefly explained and this was followed by a discussion of Doppler's principle and its application in determining radial velocity. The observations on a spectroscopic binary star, of which some 160 or 170 have been discovered, were made at as close intervals as possible and when sufficient had been obtained were combined into a curve of oscillation having as abscissa times and as ordinates velocities. The method of obtaining the period was then described, and the grouping of all observations into one period. After the curve of oscillation or velocity curve had been obtained Mr. Harper described and discussed briefly, illustrated by examples from the stars observed at Ottawa, three methods of obtaining therefrom the elements of the orbit, an analytical method due to Russell and combined geometrical and analytical methods by Lehmann-Filhés and Schwarzschild. The results obtained from the same curve by these three methods were compared, showing very good agreement.

Since the last minutes the following members have been elected :

Rev. W. A. Adcock, St. George, Beauce Co., Que.
Ralph E. De Lury, Ph.D., Dom. Observatory.
D. J. Fraser, Dom. Observatory.
Henry S. Howell, Galt, Ont.
C. W. Mitchell, 428 Lewis St., Ottawa.
Douglas S. McKenzie, Dawson City, Y. T.
R. M. Motherwell, B.A., Dom. Observatory.
Anna Ross, Strassburg, Sask.
J. M. Tribble, B.A., Dom. Observatory.

J. S. P.

AT PETERBOROUGH,

The Peterborough Section of the Royal Astronomical Society of Canada, began the second term of its initial year with a very pleasant social evening at the home of the President, Rev. Dr. Marsh, F.R.A.S., Springville. A beautiful Moon and a clear sky greeted the enthusiastic amateur astronomers and enabled them to use the telescope advantageously.

Under the guidance of Dr. Marsh the members journeyed to within a few hundred miles of the Moon and viewed at close range the monuments erected to a departed life. Excellent definition was secured and all expressed the opinion that never before had they beheld the Moon to such advantage.

A practical demonstration in photography was given and an excellent photograph of the Moon made by means of apparatus devised and constructed by Dr. Marsh. As a memento of the occasion each of the members present received by mail later, a copy. This picture is a very excellent one and Peterborough takes pride in the achievement.

Besides the regular monthly lectures, the Society has organized a series of parlor meetings at the homes of various members, where a closer study and an interchange of views and questions may take place. This has already been found of great value and the beginners have made wonderful progress.

The first of these parlor meetings was held at the home of Mr. Herbert Collier, Weller St., where, by the help of the "Planetarium" and other models of the celestial bodies, a very clear idea of the basic principles of astronomy was given. It was surprising to note by the trend of the questions asked, what slight misconceptions many bright students have.

The membership now stands at about fifty and is growing constantly. Particular care is taken in securing a membership that will prove active and studious. The list at present is very representative and reflects credit upon the Society.

The Society here has been fortunate in obtaining from the Government the loan of a telescope for the use of the members

at their homes. The kind assistance in securing this, on the part of Mr. R. F. Stupart, elicited the hearty thanks of the Society. The possession of this instrument will undoubtedly result in an activity in practical work of a worthy nature.

F. C. N. *per* H. H. C.

NOTES FROM THE DOMINION OBSERVATORY.

THE new meridian circle has been received from the makers (Messrs. Troughton & Simms) and will be installed in a few days. The aperture of the telescope is six inches, and the focal length about seven feet. The graduated circle, three feet in diameter, will be read by four microscopes. The eye end is provided with two micrometers placed at right angles to one another, that on right ascension has electrical registration.

Work on the cœlostat house began on September 26, and good progress is being made. The foundations are completed and it is expected that the superstructure will be finished within two months from the time of writing. The building will be a northerly wing of the present building, nearly 100 feet in length. A part of it is below the surface of the ground, forming a tunnel through which the beam of light will be reflected from the mirrors at the north end, into a room in the basement of the observatory. Thus will be formed what is practically a long focus telescope, which it is intended to use for observations on the Sun, including spectroscopic investigation of the sun-spots, systematic measurement of the solar radiations, etc.

At the same time, in the observatory grounds, a short distance away, is under construction a building for testing measures of length. This is 184 feet in length and 23 feet in width, and is intended to contain apparatus for testing tapes for field use, in both English and metric measure up to 50 metres. This building will not be completed until the spring.

The surveyors who have been employed on the international boundary surveys are now returning from their summer's work. For their accommodation while preparing their plans and returns of survey, rooms in the city have been rented, so as to avoid the crowding of the Observatory building, which was the cause last winter of much inconvenience.

The boundary surveys have been carried on in four distinct parts of the long line between Canada and the United States, along the 141st Meridian, which separates Yukon Territory from Alaska, along the mountains between Portland Canal and Mount St. Elias, along the southern boundary of British Columbia and along the line between the Province of Quebec and the State of New York. The field work of the survey of the southern boundary of British Columbia, which has been in progress for several years, is now completed, with the exception of a small part of the triangulation.

The Trigonometrical Survey of Canada has been pushed energetically in the Provinces of Ontario and Quebec. The angles of the triangles have been observed over an area of about 4000 square miles in Eastern Ontario. A place for a base line several miles in length has been selected near Côteau, but it has not yet been measured. Reconnaissance for further triangulation has been made of an area of about 12000 square miles, over and above the 4000 square miles observed. This area is about equally divided between Ontario and Quebec. It includes the Niagara peninsula and a considerable area around Toronto.

W. F. K.

Meridian Work and Time Service.—The new transit micrometer, made in the observatory workshop, has been in regular use for some time, though some slight alterations are still required, and the final polishing and finishing remains to be done. It was modelled for the most part after the two micrometers by Saegmüller already in use here. Among the alterations made was the arrangement of the eye-piece slide; instead of being stationary it is geared to the driving wheels so as to move at the same speed as the micrometer wire, which consequently appears fixed in the centre of the field. This makes the action in transiting a star more comparable with that of setting on a stationary point, with a consequent presumable gain in accuracy. The chronograph record is made by means of five contact strips fixed in the micrometer head, which make contact with a platinum-tipped brush. Ordinarily an automatic cut-out is arranged so

that only the four revolutions in the centre of the field are recorded; in the present case, however, there is an alternative cut-out which allows every alternate four revolutions throughout the whole field to be recorded. This enables observations to be made at some little distance from the meridian, with the possibility, if desired, of reversing the telescope between observations of the same star, thus eliminating collimation errors. Some experiments have been made on the value of this method, as well as on the advantage, if any, to be gained by increasing the number of such independent observations of each star; the results, however, have not yet been reduced.

The Time Service in the Ottawa Post Office has been in operation for some little time, including about twenty dials and a six-foot electric Tower Clock. The latter is illuminated in the same way as the one at the Observatory, by several incandescent lamps arranged to illuminate a six-foot reflector immediately behind the dial, which is of ground glass. The illumination is turned on and off automatically by drop-levers operated by a twenty-four hour wheel; this attachment was specially designed for the purpose and made in the Observatory work-shop.

Where several clocks are synchronized from a distance by one primary, as in the Observatory system, there is ever present the danger, though not a particularly serious one, of stoppage or derangement arising from intermittent trouble on the synchronization line. It is proposed to entirely eliminate this by installing in each of the five sychronized master-clocks a cut-out which, if the clock from any reason gets out of step with the synchronization current, will automatically cut off the latter. This work is in progress at the time of writing.

R. M. S.

Astrophysics.—The new correcting lens has, as given in fuller detail in the last issue, been obtained and refigured, and is now in regular use. The star image is almost entirely free from aberration, and in consequence considerably smaller and more intense. The exposure time required is considerably decreased but, owing to the very poor weather, sufficient data have not yet been

secured to give definite figures, although it is probably in the neighborhood of 30 per cent. The gain in efficiency is therefore well worth the effort required to obtain it. Moreover, this takes no account of other advantages, such as greater freedom from chance of systematic displacements of the lines, attendant upon the use of a practically perfect star image.

I am glad to report that the contract for the construction of the cœlostat house has been let, and work is now rapidly being pushed forward upon it. The cement work (foundations, piers, etc.) is practically completed, and there are good prospects of the building being ready before Christmas. Work on the ways, carriages, slow-motion mechanism, etc., for the cœlostat and mirrors is also progressing, and they will be ready to install as soon as the building is covered in.

The Littrow form of grating spectrograph, to use with the horizontal telescope, is also under construction, and actual observations should begin with the new year.

Some of the gaps in the velocity curve of ι *Orionis*, previously referred to in these Notes, which has been under observation for nearly a year have been filled and preliminary elements obtained. As it is felt that further observations may give a more satisfactory solution, fuller discussion is reserved until later. It suffices to state that the period which satisfies all observations is 29·134 days and that an eccentricity of 0·785 and a longitude of the nearer apse of 108° gives a curve agreeing the most closely with the observations.

A new method of treating geometrically the orbits of spectroscopic binaries, which has been developed by Dr. King, particulars of which will be given at the next afternoon meeting, will probably be applied in the future discussion of orbits. It is hoped thereby to considerably shorten the labor involved in obtaining the elements as well as to materially simplify the production of the ephemeris and determination of the residuals.

J. S. P.

The Seismograph.—On September 2 occurred an earthquake of considerable severity. The first preliminary tremors arrived

at $16^h\ 12^m\ 18^s$, G.M.T. The pulsations lasted for fully 2½ hours. The amplitude was considerably greater than for the Kingston, Jamaica, earthquake in January. The distance of the epicentre was computed at 7000 km. or 4400 miles. The seismogram of the above quake is a particularly pretty record, showing the billows of the long period waves of the principal portion with superposed waves of short period. It would appear that this quake occurred in the North Pacific ocean and hence was not destructive to human life as its severity might suggest. It may be remarked here, as it has not been done before, that the story or hand-writing of the seismogram has in so far been deciphered that the distance of the disturbed area, or epicentre as it is technically called, from the place of record can be fairly well deduced from the elapsed times of the arrival of the different forms of waves. The direction, however, is not so readily found. Having then a "seismic circle," by elimination and general knowledge of the seismic areas of the world the probable position of the epicentre is obtained. When one has, however, the record of three or more stations, then the position becomes one of accuracy. For the above quake the E–W and N–S pendulums responded simultaneously for the first impulse, and throughout with practically the same amplitude.

On September 23 another earthquake was recorded. The first preliminary tremors (P. T.) arrived at $21^h\ 51^m\ 50^s$ G.M.T., but were not very sharply defined as is generally the case; the maximum vibration occurred at 22^h (5 P.M. for 75° Merid. time). The disturbance lasted for fully three-quarters of an hour, and the epicentre was distant 5300 km.

In the forenoon of October 16 a very violent earthquake was recorded, the oscillations of the pendulum being about one-half the amplitude given by the great San Francisco quake. The first preliminary tremors arrived at $14^h\ 03^m\ 50^s$ G.M.T., and the maximum amplitude at $14^h\ 19^m$ G.M.T. The pulsations continued for an hour and a half, and the distance to the epicentre was about 4500 km.

Early Saturday morning, October 19, the earth got into a state of unrest and continued so for over five days, the pendulums never being quiescent. During this period an earthquake occurred on Sunday eve, just before midnight. The first preliminary tremors arrived October 21 at $4^h\ 37^m\ 00^s$ G.M.T., and the second preliminary tremors at $4^h\ 47^m\ 24^s$, giving a distance of 9400 km. to the epicentre; its destructiveness was far less, judged by the amplitude of oscillation, than that of the immediately preceding one of October 16. The pulsations lasted for over an hour and a half. During the five days of earth tremors or microseismic disturbances they increased in magnitude, reaching a maximum (absolute amplitude of 0·02 mm. Their period is so regular that the record might be described as "sawtooth" tremors. The true nature of these microseisms is not yet known, although the writer has identified rapid oscillations in a barogram with those of a seismogram, but the duration of the former would be for days, and furthermore the corresponding seismogram would not show quite that regularity of "sawtooth" tremors.

O. K.

(The above Notes were received too late for last issue).

ASTRONOMICAL NOTES.

M. Maurice Lœwy, Membre de l'Institut de France et du Bureau des Longitudes, Directeur de l'Observatoire de Paris, Membre des Académies de Berlin, St. Pétersbourg, Vienne et Washington, et Foreign Associate, R.A.S., l'Observatoire, Paris, France, and Honorary Fellow of this Society, suddenly expired on November 15th in Paris, while addressing the committees of the French Astronomical Observatories. We are indebted to him, in conjunction with M. Puiseux, for the series of magnificent photographs of the Moon that are now in our library.

"Maurice Lœwy, born April 15, 1833, was educated at the Polytechnic School and University of Vienna. Assistant in the Observatory of Vienna, 1856 to 1860. Joined the Paris Observatory in August 1860; Astronome-adjoint, 1861, August 27; Astronome titulaire, 1866, February 14; Sous-Directeur, 1878, June 27; succeeded M. Tisserand as Director, 1896, December 1. The funeral of M. Lœwy was attended by Sir David Gill and Major MacMahon, representing the Royal Society, Royal Astronomical Society, and British Association."

"Lowell Expedition to the Andes.—At the expense of Dr. Percival Lowell, Director of the Flagstaff, Arizona, Observatory, Prof. David Todd, of Amherst College, dismounted his newly-built 18-inch Clark refractor, and transported it to South America in search of a steady atmosphere, with Mars at opposition high in the zenith. The station finally selected was "Alianza," a nitrate oficina, in latitude 21° south, and located in the elevated desert pampa back of Iquique, in the desert of Tarapacá, Northern Chile. Here the night-time sky was almost invariably clear, from June 18 to August 1, during which time the planet was photographed about 7000 times, embracing a complete revolution and every part of its surface.

"Prof. Todd tells us that the much discussed canals and oases are abundantly portrayed in this unique series of planetary photographs, as many as 20 canals being sometimes caught at a single exposure, and that the crowning work of the expedition was the photographic record of numerous double canals, among them Euphrates, Thoth, Eumenthes, Dihon, Astabour, Phison, and Nilokeras."—*The Observatory*, November 1907, p. 429.

"PROFESSOR ASAPH HALL, one of the most noted of American astronomers, died on November 22 at the home of his son, Professor Angelo Hall, at Annapolis, Maryland, and was buried at Goshen, Connecticut, in the family cemetery on November 25.

* * *

"Professor Hall's career as an astronomer began at the Harvard Observatory, under William Bond, in 1857. His work there consisted mainly in the routine observatory work, but he quickly became an expert in the computation of the orbits of comets and began to show the admirable grasp of mathematical relations which later on made him an authority in problems of gravitational astronomy.

"In 1862 he entered the Naval Observatory as assistant astronomer and in 1863 was appointed professor of mathematics in the United States Navy by President Lincoln, a position he retained until retired, under the regulations, in 1891, on the completion of his sixty-second year.

* * *

"His first discovery with the 26-inch equatorial, which was of great interest, was a white spot on the planet Saturn in 1876, by means of which a new and accurate determination of the rotation period of the planet was made.

"In the summer of 1877, at the time of the near approach of the planet Mars, he made a systematic search for new satellites, which was rewarded by the most interesting discovery with which his name is connected, that of the two satellites Deimos and Phobos."—From *Science*, December 12, 1907, pp. 809–810.

J. R. C.

SUMMARY REPORT OF THE WEATHER IN CANADA. OCTOBER, 1907.

TEMPERATURE.—The temperature was supernormal from Eastern Saskatchewan to the Coast of British Columbia; normal in Southern Manitoba and very locally along the Gulf of St. Lawrence, elsewhere in Canada it was subnormal. Positive departures from the average were pronounced in Alberta, varying between 5 and 9, while negative differences of from 2 to 5 were recorded in Ontario and the greater portion of Quebec.

The following are the highest and lowest temperatures reported from the various stations:—

Yukon Territory.—Dawson, 49–14.

British Columbia.—Atlin, 54, 2; Victoria, 68, 38; New Westminster, 68, 35; Agassiz, 71, 34; Kamloops, 72, 30; Barkerville, 64, 22; Pt. Simpson, 58, 36.

Western Provinces.—Edmonton, 74, 14; Battleford, 78, 8; Calgary, 75, 20; Medicine Hat, 77, 24; Swift Current, 75, 13; Regina, 73, 11; Minnedosa, 75, 12; Winnipeg, 74, 14; Prince Albert, 69, 16; Banff, 68, 21.

Ontario.—Port Arthur, 59, 16; White River, 58, 0; Southampton, 72, 21; Parry Sound, 68, 17; Port Stanley, 65, 22; Toronto, 71, 24; Kingston, 68, 21; Rockcliffe, 68, 13; Ottawa, 64, 23; Beatrice, 67, 13; Stratford, 73, 20; Brantford, 71, 20; Sutton West, 71, 21; Lakefield, 66, 22; Port Dover, 71, 23; Lucknow, 72, 20; Paris, 73, 22; Uxbridge, 71, 17; Haliburton, 69, 11; Owen Sound, 71, 19; Stony Creek, 79, 22; Orillia, 70, 19; Port Burwell, 68, 22; Madoc, 72, 17; Cockburn Island, 69, 19; Clinton, 73, 20; Bloomfield, 67, 22; Bancroft, 70, 10; Bruce Mines, 70, 18; Lake Talon, 69, 11; Peterboro', 72, 13; Otonabee, 66, 24; Sarnia, 80, 25; Meaford, 70, 23.

Quebec.—Montreal, 67, 18, Quebec, 60, 17; Father Point, 58, 21; Sherbrooke, 66, 19; Breme, 65, 15.

Maritime Provinces.—Charlottetown, 63, 82; Chatham, 66, 22; Sydney, 64, 29; Halifax, 64, 24; Yarmouth, 62, 24; St. John, 60, 26; Parrsboro', 66, 18; Windsor, 67, 20; Summerside, 64, 25; Moncton, 65, 18; Sussex, 62, 17; St. Stephen, 65, 15; Fredericton, 67, 17.

PRECIPITATION.—The precipitation of the month differed materially over the various portions of the Dominion, in fact more so than usually occurs. From Manitoba to the Pacific Coast it was deficient to the extent of from 66 to 100 per cent., whereas from Eastern Ontario to the Gulf of St. Lawrence there was a marked excess from the average, the equivalent being 16 per cent. in the Ottawa Valley, increasing to the large amount of 102 per cent. in the Gaspé Peninsula. In Ontario, over the greater portion of the Province, the precipitation varied considerably with the district, some localities recording a positive departure and others a negative. In the Maritime Provinces the departures from the average were unimportant except in Cape Breton where a considerable excess was experienced.

NOVEMBER, 1907.

TEMPERATURE.—The mean temperature of November was higher than the average throughout the Western Provinces and in British Columbia, the widest departure about 10° occurring in Alberta and Saskatchewan. In Ontario, east of Lake Huron and north of Lakes Erie and Ontario, the departure was very generally negative by one or two degrees, while in Quebec and the Maritime Provinces the mean of the month ranged from just average to 2° above. From Ontario to the Maritime Provinces there were but two fairly pronounced cool periods, namely, from the 12th to 16th, and again during the last few days of the month.

The following are the highest and lowest temperatures recorded at various stations:—

British Columbia.—Atlin, 44, 8; Victoria, 58, 36; New Westminster, 57, 34; Barkerville, 44, -2; Kamloops, 62, 20; Agassiz, 69, 31.

Western Provinces.—Dawson, 26, −30; Edmonton, 56, 8; Battleford, 58, 4; Prince Albert, 51, 4; Calgary, 61, 14; Medicine Hat, 64, 7; Swift Current, 60, 2; Regina, 57, 0; Minnedosa, 51, −10; Winnipeg, 52, −12.

Ontario.—Port Arthur, 48, 6; White River, 44, −10; Southampton, 54, 18; Parry Sound, 51, 8; Port Stanley, 53, 17; Toronto, 51, 18; Kingston, 53, 13; Rockcliffe, 48, 0; Ottawa, 50, 14; Bruce Mines, 50, 3; Clinton, 53, 6; Otonabee, 50, 15; Uxbridge, 49, 14; Paris, 53, 16; Lucknow, 53, 13; Port Dover, 52, 19; Peterboro', 50, 8; Huntsville, 48, 8; Cottam, 54, 16; Madoc, 51, 10; Port Hope, 52, 16; Bancroft, 47, 2; Beatrice, 48, 6; Owen Sound, 52, 18; Brantford, 54, 15; Stony Creek, 55, 19; East Toronto, 49, 14; Sarnia, 55, 21; Meaford, 53, 19; Port Burwell, 55, 20; Stratford, 52, 10; Welland, 58, 20; Wallaceburg, 55, 18.

Quebec.—Montreal, 52, 16; Quebec, 53, 11; Father Point, 53, 12; Brome, 56, −5; Sherbrooke, 56, 6.

Maritime Provinces.—Charlottetown, 56, 20; Chatham, 58, 14; Sydney, 61, 20; Halifax, 57, 20; Yarmouth, 59, 20; St. John, 57, 19; Fredericton, 57, 12; St. Stephen, 56, 13; Sussex, 62, 12; Port Hastings, 67, 24.

PRECIPITATION.—The precipitation was heavy over the Lower Mainland of British Columbia and comparatively light on the Upper Mainland, 13·4 inches were recorded at Vancouver, 8·4 at Agassiz, and but 0·5 at Kamloops. In the Western Provinces it was almost *nil*, ranging from just a few snow flurries in Alberta to an aggregate of 0·15 rain and about 4 inches of snow in Manitoba. Over the greater portion of Ontario the precipitation was part rain and part snow, but chiefly the former. It was fairly generally in excess of the average except near the shore lines of the lakes, where there was a small deficiency. In Quebec and the Maritime Provinces departures from the average amount were not pronounced except in Prince Edward Island, where the quantity recorded was much below average and decidedly less than in New Brunswick and Nova Scotia.

DEPTH OF SNOW. At the close of the month there was a light covering of snow over the whole of Quebec and over the larger portion of Ontario, while in parts of this latter Province east and north of the Georgian Bay as much as 12 inches was reported. In northern New Brunswick there was also a light covering, but further south and including Prince Edward Island and Nova Scotia there was but a trace here and there. In Manitoba there was from half an inch to 2 inches, but further west all the prairie lands were quite bare as were also the lower levels in British Columbia.

METEOROLOGICAL OFFICE,
TORONTO,
DECEMBER 12, 1907.

INDEX TO VOLUME I.

A

Abbot, C. G., Variation in Solar Radiation, 6.
Adams, W. S., Researches on Sunspots, 3.
Adapting a Universal Spectroscope, *J. S. Plaskett*, 104.
Asteroids, Researches on, 9.
Astronomical and Astrophysical Society of America, 67.
Astronomical Notes and News, 72, 74, 140, 209, 269, 326, 376.
Astronomical Society of the Pacific, 268.
Astronomy as a Science, *W. F. King*, 22.
Astronomy of Shakespeare, *John A. Paterson*, 273.
Astronomy of the Bible, *J. E. Maybee*, 166.
Astrophysics, notes on, 69, 135, 205, 264, 372.
Audibility of the Aurora, *W. F. King*, 193.
Aurora and Magnetic Disturbance, *R. F. Stupart*, 38, 43.
Aurora, Audibility of, *W. F. King*, 193.
Aurora, Remarkable case of August 7, 1906, 42.
Aurora, The, *F. A. McDiarmid*, 201.

B

Bequest to University of Toronto, 76.
Bible, Astronomy of the, *J. E. Maybee*, 166.
Boundary Surveys, Notes on, 133.

C

Ceraski, on Stellar Magnitude of Sun, 6.
Chant, C. A., Astronomical and Astrophysical Progress, 2.
Work and Improvements at Lick Observatory, 246.
Clerke, Agnes Mary, by *Elsie A. Dent*, 81.
Comets discovered in 1906, 14.
Comet 1907 *a*, 139; 1907 *d*, 209, 213, 269; 1907 *e*, 327.
Cosmical Aspects of Radioactivity, *E. Rutherford*, 145.

D

Dawn of Astronomy, *Joseph Pope*, 366.
Dawson, W. B., Variation in Tides in Different Regions, 213.
Denning, W. F., Observations on Jupiter, 8.
Double Stars, New Discoveries, 11; Notes on, by *T. E. Espin*, 186, 333; Observed at Lick Observatory, 258.

E

Earthquakes of 1906, *R. F. Stupart*, 65.
Eclipse of August 30, 1905, 3; of January 1907, 5; of January 1908, 5, 254.
Elvins, A., Thoughts on Meteorology and Terrestrial Magnetism, 125; Remarks on the Tides, 129.
Equipment, Improvements in, 15.
Errata in *Handbook for 1907*, 139.
Errors in Transit Observations, *R. M. Stewart*, 228.
Espin, T. E., Notes on Double Stars, 186, 333.

Eta Piscium, period of, 69.

F

"Flash" Spectrum, *S. A. Mitchell* on, 4.

G

Geodetic Measurements, 14.
Geodetic Survey, Notes on, 135.
Globular Lightning, Case of, 44.
Gore, J. E., Number of the Stars, 10.
Granulation of Sun's Surface, *Hansky*, 5.

H

Hale, G. E., Programme of Research, 2.
Hall, Maxwell, Observations on Zodiacal Light, 12.
Hamilton, James Cleland, death of, 65.
Handbook issued by the Society, 18.
Hansky, Studies of Solar granulation, 5.
Harper, W. E., Spectroscopic Binary α Draconis, 237; θ Aquilæ, 357; Orbits of Stellar Systems 367.
Hassard, A. R., Transit of Mercury, 362.
History of Astronomy, *W. F. King*, 364.
Howell, D. J., Applications of Photography to Astronomy, 197.

I

Introduction, 1.
In Memoriam (A. M. Clerke), *T. E. Heath*, 81.

K

Kapteyn, Professor, Work on the Stars, 10.
Kavanagh, I. J., Stoneyhurst College Observatory, 291.
Keeler Memorial Volume, 261.
King, W. F., Astronomy as a Science, 22; Audibility of the Aurora, 193; Theory of Least Squares, 347; History of Astronomy, 364.
Kirschmann, A., Transmutation of the Elements, 337.
Klotz, Otto, On Planetary Motion, 130.

L

Least Squares, Theory of, *W. F. King*, 347.
Lick Observatory, Work and Improvements at, *C. A. Chant*, 246.
Loudon, W. J., Tides on Lake Huron, 181.
Lowell, P., Researches on Mars, 8.

M

Magnetic Disturbances and the Aurora, *R. F. Stupart*, 38, 43.
Magnetic Disturbances Recorded at Agincourt Observatory, *R. F. Stupart*, 122.
Marsh, D. B., The Telescope, 188.
Maybee, J. E., Astronomy of the Bible, 166; Lunar and Terrestrial Craters, 127.
Medal, The Society's Gold, 17.
Meetings of the Society, at Toronto, 64, 127, 195, 323, 362; at Ottawa, 61, 129, 199, 364; at Peterborough, 203, 368.
Mellish's Comet, 1907 *e*, 327.
Mendeléeff, D., Facsimile Letter from, 75.
Mercury, Transit of, *A. R. Hassard*, 362.
Meridian Circle at Lick Observatory, 259.
Meteoric Iron, Presentation to the Society, 197.
Meteor, Extraordinary case by *C. B. Anderson*, 13.
Meteors and Comets, Researches on, 13.
Millochau and Stefanik, Spectrum of Corona, 5.
Mira Ceti, Spectrum of, *J. S. Plaskett*, 45.
Mitchell, S. A., on "Flash" Spectrum, 4.
Moissan, Temperature of the Sun, 6.
Moon, its Origin, *(Pickering)*, 138.
Moon, Researches on the, 7.
Muir, R. S., Use of Esperanto, 127; Presentation to the Society, 363.
McDiarmid, F. A., The Aurora, 201.
McEachren, A Remarkable Aurora, 42.

N

Nebula, New one in Scorpio *(Barnard)*, 12.
Notes on Double Stars, *Espin*, 186, 333.

Notes from the Dominion Observatory, 69, 133, 205, 264, 370.

O

Optics of the Telescope, *J. S. Plaskett*, 131.

Orbits of Stellar Systems, *W. E. Harper*, 367.

Ottawa Section, Organization of, 60.

P

Papers and Lectures in 1906, 20.

Paterson, John A., Astronomy of Shakespeare, 273.

Photometry of the Stars, (Potsdam), 10.

Pickering, W. H., Investigations on the Moon, 7.

Planets, Researches on the, 8.

Plaskett, J. S., Adapting a Universal Spectroscope, 104; Optics of the Telescope, 131; Spectrum of *Mira Ceti*, 45; Star Image in Spectroscopic Work, 297, 365.

Pope, Joseph, The Dawn of Astronomy, 366.

Progress in Astronomy and Astrophysics in 1906, 2.

Pyramid, A Few Facts about, *J. F. Sharpe*, 196.

R

Radioactivity, Cosmical Aspects of, *E. Rutherford*, 145.

Roberts, A. W., Study of β Lyræ, 11.

Rutherford, E., Some Cosmical Aspects of Radioactivity, 145.

S

Saturn, Knots on its Ring, 326.

Seismograph, Notes on the, 70, 206, 265, 328, 373.

Shakespeare, Astronomy of, *John A. Paterson*, 273.

Sharpe, J. F., Facts about the Great Pyramid, 196.

Special Lectures delivered in 1906, 17.

Spectroscopic Binary α Draconis, *W. E. Harper*, 237, θ Aquilæ, 357.

Spectrum of *Mira Ceti*, *J. S. Plaskett*, 45.

Star Image in Spectrographic Work, J. *S. Plaskett*, 297, 365.

Stewart, L. B., Variation of Latitude, 363.

Stewart, R. M., Errors in Transit Observations, 228; Time Service at the Dominion Observatory, 85.

Stonyhurst College Observatory, *I. J. Kavanagh*, 291.

Stupart, R. F., Earthquakes of 1906, 65; Magnetic Disturbance and the Aurora, 38, 43; Magnetic Disturbances Recorded at Agincourt, 122.

Summary Report of the Weather in Canada, 77, 141, 210, 270, 329, 378.

Sun, Review of Researches on, 2; Forms of Setting Sun, plate VI, opp. p. 273.

T

Telescope, the, *D. B. Marsh*, 188.

Theory of Least Squares, *W. F. King*, 347.

Theta Aquilæ, Preliminary Orbit of, *W. E. Harper*, 357.

Thoughts on Meteorology and Terrestrial Magnetism, *A. Elvins*, 125.

Tides on Lake Huron, *W. J. Loudon*, 181.

Tide, Variation in Different Regions, *W. B. Dawson*, 213.

Time Service at Dominion Observatory, *R. M. Stewart*, 70, 85.

Time Service, Notes on, 136, 207, 371.

Transit Observations, Errors in, *R. M. Stewart*, 228.

Transmutation of the Elements, *A. Kirschmann*, 337.

Treasurer's Report for 1906, 21.

U

University of Toronto and the Society, 17.

V

Variable Stars, Researches on, 10.

Variation of the Tide in Different Regions, *W. B. Dawson*, 213.

Variation of Latitude, *L. B. Stewart*, 363.

Vogel, Hermann Karl, 360.

W

Weather in Canada, Summary Report of, 77, 141, 210, 270, 329, 378.

Work of the R. A. S. C. Reviewed, 16.

Z

Zodiacal Light, Researches on, 12.

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